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THE LOGIC OF DISCOVERY

IT IS CONCEIVABLE that in the nature of things the mind is unable to comprehend its highest movements with clearness. If the act of discovery or invention, in its rarer and more profound phases, is, as it seems to be, one of the highest experiences of the mind, it would then be natural to expect that the mind itself could not explain such an act of invention or discovery. If any portion of the mental processes should thus lie beyond the reach of scientific analysis, one would have a domain in which precision of thought could not be attained. This looks too much like a mystery to be accepted without repugnance. But there seems to be a real difficulty in supposing that the mind can explain, or comprehend clearly, all of its acts. If so, it could explain also the act of explanation, and then this act in turn, and so on, apparently with an infinite regression. Whether such impossible infinite regression can or cannot be avoided, it is clearly a conceivable possibility that certain mental processes cannot be fully apprehended by the experiencing agent, so that one must not assume in advance that the mind can certainly comprehend clearly all of its own movements.

But this conceivable possibility need not at all affect the development of a logic of discovery in the sense of a logic by which one infers from the known to that unknown which hitherto has not been apprehended or suspected. It is with the laws of such inference that a logic of discovery

will be concerned. The data from which one starts and the conclusions reached through the use of these laws will be clearly apprehended in their relation to the process; and the completed act with its several steps can be held up, as it were, before the mind for inspection and analysis. The processes of thought in such discovery may be studied objectively after the act by means of the remembered steps of the inference; and the laws of such inference may be successfully investigated.

But it must not be assumed in advance that all the processes of discovery are carried forward by means of a logic—the inference from known to unknown. It is conceivable that any logic of discovery will necessarily leave out of account some of the most characteristic acts of discovery. One might prefer to avoid this conclusion, but one must be prepared to face the possibility. As J. A. Thomson has said: “It may be that the imaginative brooding suggests a solution in some way that we do not at present understand—life is essentially creative; it may be that there is a more or less unconscious cerebral experimenting; it is certain that letting the mind play among facts has often led to magnificent conclusions. It seems that the solution is often reached first and the proof supplied afterwards.” There is the conceivable possibility of an actual creative activity of the intellect which is not suitably analyzed in terms of any logic of inference from the known to the unknown. It is clear that many of us wish to have our universe so tidy that nothing actually novel could happen in it, that every event should be a mere consequence of what had preceded it. But there are others who would not object to the surprises of true novelty, who indeed would be pleased with them rather than disconcerted by them. But the matter is controlled by the wishes of neither group. The question is one of fact, hard to be ascertained perhaps, and not one of opinion.

In this also, whatever the truth may be, it does not affect the development of a logic of discovery. For such creative acts, if they really take place, are outside of the category of inferences from the known to the unknown; and hence lie outside of the domain of a logic of discovery.

To our definition of the logic of discovery as the science of inference from the known to that unknown which hitherto has not been apprehended or suspected, we may add a few remarks as to what it is not by way of more clearly delimiting the meaning of the term. In the first place it is not necessarily a logic of demonstration. One may discover a truth by means of a definite process of inference which leads forward by well-defined steps to a clearly ascertainable proposition without carrying with it a demonstration of that proposition. This is often done by the mathematicians in important ranges of investigation. An actual demonstration of the result thus heuristically attained is then frequently given in a *de novo* argument. If some particular type of inference which is thus often successful should turn out to be always so, in the sense of never leading to false conclusions, one would suspect that a more careful analysis of it would reveal the fact that it could be put into the form of an actual demonstration. But some of these heuristic methods, which are successful in yielding true conclusions when they are sagaciously employed, sometimes lead one also to formulate propositions which turn out to be false. They give a method of discovery which must be checked by a subsequent demonstration of the results. Such a method affords an example of a logic of discovery which is not a logic of demonstration.

We have already considered the possible existence of a creative method of invention which cannot be treated by a logic of discovery, the latter being confined to inferences from the known to the unknown. Such a logic, then, does not take account of all things, even of all important things,

relative to discovery. Some of these may belong to the psychology or the physiology of the investigator. The logic of discovery has to do only with certain laws of inference. If we should think of the body of known truth in a given field as being inclosed in a region beyond the boundary of which lies what is unknown, it might be an important question whether one is most likely to be able to break over this boundary if he approaches it on a gradual spiral path taking him through a large part of what is known already or along a radial path quickly bringing him squarely against the boundary; such a question, whether important or not, would have nothing to do with a logic of discovery. It belongs rather to the psychology of investigation relative to the particular domain of truth. The logic of discovery has to do only with the laws of inference from the known to the unknown.

When we notice to what extent discoveries in science appear to be made in unforeseen ways and how often an accidental juxtaposition of thoughts leads to the detection of something new, we feel that it would be hazardous to miss one link from the chain of scientific progress or leave out of our thought even the least inkling as to what may be a successful logic of discovery. We have not yet learned how to systematically explore new territories of thought. We can only look over the field at random and hope to find here and there a pearl of great price. In the absence of a guiding logic of discovery we have no systematic method of procedure.

Francis Bacon has emphasized this matter, saying, "So it cannot be found strange if sciences be no farther discovered, if the art itself of invention and discovery hath been passed over. That this part of knowledge is wanting, to my judgment standeth plainly confessed; for first, logic doth not pretend to invent sciences, or the axioms of sciences, but passeth it over with a '*cuique in sua arte cre-*

dendum.'” He says further: “So it should seem, that men are hitherto rather beholden to a wild goat for surgery, or to a nightingale for music, or to the ibis for some part of physic, or to the pot lid that flew open for artillery, or generally to chance, or anything else, than to logic, for the invention of arts and sciences.”

Descartes also speaks much to the same tenor; he says: “I found that, as for logic, its syllogisms and the majority of its other precepts are of avail rather in the communication of what we already know, or even . . . in speaking without judgment of things of which we are ignorant, than in the investigation of the unknown.” As to places where he might find correct reasonings, Descartes writes: “For it occurred to me that I should find much more truth in the reasonings of each individual with reference to the affairs in which he is personally interested, and the issue of which must presently punish him if he has judged amiss, than in those conducted by a man of letters in his study, regarding speculative matters that are of no practical moment, and followed by no consequences to himself.” But the primary source of that method which is so clearly described in Descartes’ classic *Discourse* is in mathematics. He gives the following account: “The long chains of simple and easy reasonings by means of which geometers are accustomed to reach the conclusions of their most difficult demonstrations had led me to imagine that all things, to the knowledge of which man is competent, are mutually connected in the same way, and that there is nothing so far removed from us as to be beyond our reach, or so hidden that we cannot discover it, provided only that we abstain from accepting the false for the true, and always preserve in our thoughts the order necessary for the deduction of one truth from another.”

Aristotle was almost entirely concerned with establishing what had been conceived already or of refuting

error, but not with solving the problem of the discovery of truth. Now and then, in reading his organon, one feels that he has almost sensed the nature of this problem, only to find that he lapses immediately into a discussion of the logic of demonstration. He thinks of confirming truth rather than of finding it.

The Renaissance gave birth to the demand for a new organon, "a scientific method which shall face the facts of experience and justify itself by its achievement in the reduction of them to control." Bacon called for the overthrow of the dominant system, for a new beginning, for recourse to nature, for induction in a safeguarded form, for experiment, for a logic of discovery. He objected to the syllogism as constraining assent where we want a control over things. But he did not succeed in his great object of founding a logic of discovery. "It has been pointed out, and with perfect justice, that science in its progress has not followed the Baconian method, that no one discovery can be pointed to which can be definitely ascribed to the use of his rules." Descartes' doctrine of clarity as the supreme criterion for a method of discovery does not come to grips with the real problem. His extension of the method of mathematics into a general method of reasoning and discovery is not adequate to the varied needs of the investigator.

So goes the story through the whole history of logic. The developed systematic logic is a logic of demonstration. Whewell saw that "science advances only in so far as the mind of the inquirer is able to suggest organizing ideas whereby our observations and experiments are colligated into intelligible system"; but he could give no direction for the capture of these organizing ideas. In the article on induction in the *Encyclopaedia Britannica* we read: "The most important faculty in scientific inquiry is the faculty of suggesting new and valuable hypotheses. But

no one has ever given any explanation how the hypotheses arise in the mind; we attribute it to 'genius,' which, of course, is no explanation at all. The logic of discovery, in the higher sense of the term, simply has no existence. Another important but neglected province of the subject is the relation of scientific induction to the inductions of everyday life. There are some who think that a study of this relation would quite transform the accepted view of induction. Consider such a piece of reasoning as may be heard any day in a court of justice, a detective who explains how in his opinion a certain burglary was effected. . . . What the detective does is to reconstruct a particular crime; he evolves no general principle. Such reasoning is used by every man in every hour of his life; by it we understand what people are doing around us, and what is the meaning of the sense-impressions we receive."

Two distinct causes may contribute to the failure to produce a logic of discovery, one having to do with the nature of the mind and the other with the assumed nature of the logic.

It is conceivable, as we have said, that the primary acts of discovery should be so largely or so thoroughly creative in their character that no science of inference from the known to the unknown can be developed; or, if it can be developed in part, that it cannot be adequate except in a very restricted range. If the process is a creative one then it would probably be agreed that it is a process whose movements cannot be predicted or analyzed into cause and effect, so that the ascription of the results solely to creative action would have the effect of closing inquiry into the nature of the process. On the other hand, if the process is truly creative in its nature it is clear that we come to no better understanding of it by shutting our eyes to that fact. But if there exists at all such a method of procedure lying beyond the reach of systematic analysis,

it appears to belong to the greater minds and to their rarer moments. In large part the method of discovery seems to belong to a logic of discovery. Leaving unsettled the question as to whether any discovery is truly creative in character, one may justly proceed to ascertain to what extent the methods of discovery can be described in terms of a science of inference. If in the nature of things we are kept away from the goal of an adequate logic of discovery, we shall nevertheless in this way get as near to it as is possible for us. We shall, however, not fail to remember the fact that we are investigating only one of two conceivable methods of discovery.

It is evident that Bacon conceived of the logic of discovery as a unit which is scarcely separable into parts. Descartes obviously held the same view in a different form. Such seems to have been the opinion of most of those who have sought to develop the subject. In several places, I have met the term logic of discovery but seldom or never the notion of logics of discovery. It is conceivable that the logic of discovery is not one in the sense of something indivisible, but that it is relative to the field of investigation or the point of view so that one should not speak of a logic of discovery in any absolute sense, but only of such a logic as relative to a given discipline or a given goal of investigation.

The usual failure to divide the problem into the parts thus suggested has, I believe, been a chief hindrance to the development of the logic of discovery. The fact that the logic of demonstration is a unit, being the same whatever the field of investigation, has led to a too ready acceptance of the view that a logic of discovery should also be a unit.

Discovery itself may be relative to the point of view of the investigator. Through the changes induced in the philosophy of science by recent advances in physics the

concept of explanation has undergone a considerable modification through the formation of a new list of basic fundamental elements in terms of which explanation has to be made. Of this change Rougier (*"Philosophy and the New Physics,"* pp. 146-147), has written as follows:

"In former days a physical phenomenon was explained by reducing it to the principles of classical mechanics, by giving to its laws the form impressed by Lagrange on the equations of dynamics. To explain a phenomenon today is to give it a statistical explanation, by regarding it as the resultant of a very large number of underlying phenomena governed by the laws of chance. . . .

"Thus not only do the most fundamental categories of our mind, those of space, of time, of causality, pass through an evolution with the progress of science but the same holds even for the concept of intelligibility. To explain a phenomenon is, for primitive man, to interpret it anthropomorphically by a supernatural agent endowed with psychological life in his own image; for a scholastic it is to explain it by ultimate causes; for Maxwell it is to deduce it from the principles of mechanics; for Gibbs and Boltzmann it is to account for it by the calculus of probabilities, by starting from a system of elements subject to given conditions. Human reason is not '*une et entière en chacun*' as Descartes taught. It varies with the abstract or concrete nature of our thought, and in proportion as, on contact with experimental facts, the adaptation of our mind to nature becomes progressively realized."

Now when one modifies the meaning of such a fundamental thing as intelligibility, or explanation, he changes his point of view so radically that he will, in his investigations, look for quite different things from those for which he would otherwise look. There is a great difference between the way of work of one who expects to find the inner secrets of phenomena and that of one who supposes that he

is only to get some convenient shorthand way of expressing the relations of phenomena without any approach to their ultimate explanation. One can set himself to find only those sorts of explanation which he deems to be possible. He may find other things by accident as it were. But he cannot seek them systematically. His logic of discovery, the way he infers in fact as opposed to the way in which he infers perfectly, varies profoundly with changes in his point of view and especially in his view as to what constitutes explanation.

When one conceives a definite law of progress from lower to higher methods of thought, as Comte did in connection with his law of the three states, he will carefully direct his own thought towards what he conceives to be the higher. Indeed, in a case so well marked as that of Comte, he will avoid entirely the methods which are conceived to be of the lower sort and will undertake to carry forward his investigations solely by means of what he conceives to be the higher method. In this way an abstract ideal of excellence, when formed in accordance with a classification of method as more primitive or more secure, necessarily dominates the order of procedure in demonstration. The logic of discovery is a function of the ideal of excellence in different sorts of truth; it depends on the point of view.

With the conception of scientific explanation which is prevalent in our times it would be quite impossible for one to proceed as Descartes proceeded. One of his editors says of him: "Refusing to let himself be hindered by lack of adequate information, he thought out what the constitution of the world and man must be if they were to be clearly understood." Descartes conceived of clearness of thought as a criterion for truth and was convinced that God had arranged things so that true knowledge is possible. Then if it were true that the ideal of clearness of thought could

be attained under only one conception of the nature of God and the world and man, then this must be the valid conception of these things. Out of the mind itself and the ideal of true thinking which was imposed upon it, he believed that some of the most momentous conclusions of science could be deduced without any experimental evidence. The necessary type of scientific explanation could be deduced by considerations having to do with the mind itself. He realized that experience is necessary for details; but the fundamental terms in which the explanation must be made he deduced by means of his ideal of clearness of thought.

The logic of discovery which is implicit in this type of argument makes no strong appeal to scientific thinkers of our day. It depends upon a conception of nature and thought too far removed from what is now current. But the example serves to enforce the fact that such a logic may be relative to the ideal elements in the point of view.

Before one can proceed to a detailed development of any logic of discovery it is important that he shall determine in what part of the thinking process is to be found the essential step of discovery. It is clear that it is not in the proof of a proposition once conceived, even though with uncertainty; the latter requires only the use of the logic of demonstration, whether the conjectured proposition is established or shown to be false. The essential step is in a much more original act. It is in the formation of the conjecture itself or goes back even farther to the formation of the hypothesis out of which comes the proposition to be tested, whether by experiment or by reasoning. It may even be found in a more remote place in the process of discovery than this, its chief element resting in a principle partaking somewhat of a metaphysical nature (as in the general principle of relativity), or in an ideal of a purely abstract character (as in Descartes' doctrine of clar-

ity). It is as if the mind were seeking to impose itself upon nature, insisting that whatever explanations we may finally adopt they shall be such as satisfy the requirements of a norm set up by the mind itself. Certain of these demands may be impossible of realization. One then constructs a norm of a modified sort. The essential step in discovery is in the construction of definite hypothesis in the form of a particular or a general law or proposition and in the formulation of a principle or norm lying back of the hypothesis and contributing effectively to giving it existence.

Galileo informs us that he discovered by reason the law of distance for falling bodies and that he afterwards verified it by experiment. The Copernican assertion of the motion of the earth is neither a deduction of the pure reason nor a datum of experience but an hypothesis which has been verified. Kepler can tell us the precise date on which he conceived correctly the relation of periodic times in planetary motion though it was more than a month afterwards before he succeeded in verifying the law by detailed computations. The law of gravitation offered itself clearly to Newton's thought in 1666, but was temporarily discarded from lack of agreement with recorded observations, to be revived and accepted later when more accurate observations were available for a better check. He was so agitated over the possibility that these new observations would verify his theory that he got one of his friends to undertake the necessary computations for him because in his emotional excitement he did not feel capable of doing it himself. The laws of nature, in the absence of sufficient experimental evidence to prove them, are often conceived through a happy combination of thoughts in the mind of the investigator. Innumerable useless combinations are passed over and the vital ones rise to consciousness to bring new truth to light.

An instructive failure to realize the importance of new hypothesis in physical science is brought out by the following two paragraphs taken from the "Register" of one of our leading American universities where they appeared regularly for a decade and a half overlapping the end of the last century and the beginning of this:

"While it is never safe to affirm that the future of Physical Science has no marvels in store even more marvelous than those of the past, it seems probable that most of the grand underlying principles have been firmly established and that further advances are to be sought chiefly in the rigorous application of these principles to all the phenomena which come under our notice.

"It is here that the science of measurement shows its importance—where quantitative results are more to be desired than qualitative work. An eminent physicist has remarked that the future truths of Physical Science are to be looked for in the sixth place of decimals."

This conception of the state of physical science seems to have had considerable currency in the earlier nineties. Since then a veritable revolution has taken place. New theories have sprung up and have manifested remarkable vitality. An eminent physicist has advised the young men to try all sorts of "fool experiments" on the ground that there is no way to anticipate what remarkable things may thus be brought to light. After a period in which successful hypotheses were seldom formed there has come one when new and even startling hypotheses have followed one after another with bewildering rapidity, and physical science has taken such a leap forward as has been witnessed only two or three times in its history.

This emphasizes the importance of the place of hypothesis in the process of discovery.

The logic of demonstration is by definition only that sort of logic which compels assent to the conclusion when

the premises are granted. It is this logic by which one is always to establish those results which are to be made secure in virtue of their logical dependence on results which are already known to be secure. It is the universal method of the mathematician when he sets forth for others the proofs of the truths discovered by him. In the natural sciences it is often true that one must start from principles which are only probably, or even only conjecturally, true. There is always the possibility that some new phenomenon will be brought to light not in agreement with the principles already accepted, so that one never establishes precise results with compelling logic. There is always the need for an experimental test. In a certain part of mathematics this is not so, namely; in that part in which the doctrine advanced gives rise admittedly to a body of results that follow from given postulates which are accepted.

This marked difference between mathematics and physical science is not altogether so universal as has sometimes been supposed. In some fields, as in that of the theory of numbers for instance, we are dealing with a set of objects which we assume ourselves to know so thoroughly that our basic propositions are not so much postulates as the statement of known properties, as of the positive integers in the field mentioned. It is conceivable that as a matter of fact we do not know the positive integers well enough for this; and that, on the basis of the initially accepted properties, we may be led to some result not holding for all positive integers. We should then be compelled to modify our statement regarding our theorems and say merely that they are true for those entities which satisfy our basic propositions. We have such confidence, through the result of previous experience, that such a breakdown is not going to ensue that we proceed without any systematic experimental verification of our results. We do, however, subject them often to the test of more or less random

numerical verification; and this is in many respects similar to the experimental test in the laboratory of some conclusion in natural science obtained from theoretical considerations.

If it is objected that we do not develop the theory of positive integers from our clear conception of their basic properties but from an assumed basis of postulates, the answer is that the latter is indeed the theoretically satisfying form, but that investigators and expositors in the theory of numbers have for the most part proceeded in the way we have indicated from propositions the truth of which they have granted without question, and have not thought of their work as giving the consequences of certain postulates so much as yielding veritable properties of clearly perceived existent entities. If it is objected that this is not a perfect procedure, it may be said in reply that it is the procedure which has actually been employed. The theory of numbers, as a matter of historical fact, has been developed from certain propositions concerning numbers the truth of which one seems to ascertain immediately from his acquaintance with integers either through experience of them or through the invention or creation of them by the human mind.

These basic truths are closely analogous to the laws of the physicist. Perhaps one has a right to accept them with greater confidence than is legitimate for the physicist in his more complex domain; but the ground of the confidence seems to be of essentially the same sort, the increased confidence being due to our fuller knowledge of positive integers than of electrons for instance. Whether this fuller initial knowledge is due to the fact (if it is a fact) that the human race created integers or is due to a longer and more intimate experimental acquaintance with integers does not seem to alter the essential character of this initial knowledge. If we accept the principles of an exact logic

and agree to a given set of basic propositions, then we necessarily accept the results which flow from these propositions by means of these logical principles. Thus we are accustomed to begin the development of the theory of positive integers, not from a body of assumptions but from a body of propositions which we agree are true of positive integers—for instance, the proposition that the larger number A of two positive integers can be written as a sum of two terms one of which is an integral multiple of the smaller number B and the other is zero or a positive integer less than B . These we take not so much for postulates as for true propositions from which we begin our argument.

There are two possibilities concerning the character of this knowledge. Either we have it by an immediate insight or intuition of its truth; or we have attained it on some sort of experimental basis. If it is by the former, then we have no suitable means of knowing the validity of our insight; if it is by the latter, then we cannot be said to have tested the matter fully until we have examined every aspect. But to examine every aspect of it we shall have to verify every logical consequence of the originally accepted propositions. Then we can never be said to know fully the truth of a proposition which we have derived logically unless we subject it to some sort of experimental test, provided that that truth is not merely one which asserts the logical connection of propositions.

Hence, either from the lack of complete certainty of our insight or of the full reach of our experience we are in the position of being short of absolute logical certainty even for our propositions about positive integers. But we have so frequently verified our results in the past that we have attained to an emphatic confidence that they will be verified in the future. Our experimental evidence is great enough to give us a strong feeling of security. And yet it may be observed that workers in number theory still seem

to feel a certain satisfaction in exhibiting numerical verifications of their more abstruse theorems.

This seems to me more like the experimental verification of the natural scientist than is usually supposed. The mathematician does not feel so keenly the need of it as the physicist; but is not this confidence, after all, due primarily to the mathematician's previous experience of almost constant success whereas the physicist has more often reached wrong conclusions, due presumably to the greater intrinsic difficulty of his subject matter? Even the physicist, as we have already seen, has passed through stages in which he was almost absolutely confident of his principles and was looking around only to find means to get the right figure in the sixth decimal place. In biology there appears to be almost the same feeling of absolute certainty that things have come to their present state through some process of evolution which is not merely one cycle in an unending sequence of repeated cycles.

In the progress of knowledge we are concerned both with the logic of demonstration for the firm establishment of truths once suspected and with the way or means by which one may come in the first place to formulate a proposition and to suspect its truth. The question also arises as to whether a sure process of inference from the known to the unknown exists—that is, whether there are well defined characteristic processes, imbued with full logical rigor, by which one may pass directly from the known to the unknown in such a way that in the very passage to the new truth there is inherent the forcible logical demonstration of that truth. Or, should we seek rather some sort of heuristic logic by which one comes first to formulate a proposition whose truth he suspects, while the demonstration of it is to be sought later by more secure processes?

It seems certain that the former alternative is not realized. There is no secure logic of discovery different from

the logic of demonstration. Whatever process of reasoning ends with a new truth, demonstrated as it is attained, is carried out only by a secure logic of demonstration. This does not mean that the latter is never a logic of discovery; in fact, it is often this. Many truths which assert merely the logical dependence of propositions are attained by a logic of demonstration—especially when the propositions are conceived in their abstract form. And not a few others are also derived in this way. Maxwell's prediction of the pressure of light resulted from a truth discovered by a logic which carried with it a demonstration of the fact that this truth is a consequence of accepted laws of physical phenomena. The most striking recent instance of this sort of discovery is that of the bending of a ray of light in a strong gravitational field, as predicted by Einstein in his general theory of relativity.

It appears to me that the use of a logic of demonstration for the purposes of discovery does not afford a typical instance of the logic of discovery. If there is any point to considering the latter at all it is because it has, in important instances at least, characteristic qualities which are worthy of investigation. Accordingly we turn now to a further consideration of the question of the existence of some sort of heuristic logic by which one comes first to formulate a proposition whose truth he suspects, while the demonstration of it is to be sought later by more secure processes. That such processes of inference exist has certainly been recognized since the time of Aristotle. Analogical reasoning is of just this sort and so is the conclusion from the particular to the general. But the problem which we have in mind is not so much that of the general principles of probable but insecure inference, as of that which arises in consideration of some such question as the following:

Have the particular sciences certain heuristic logics and do these vary, in whole or in part, as we pass from one particular science to another? This question forces upon our attention another, namely, the question as to whether all logic is one or whether logic is relative to the field or the subject matter to which it is to be applied. The foregoing separation of logic into two parts seems important here; and we should probably press a two-fold question: Is the logic of secure demonstration one, the same in all ranges to which it may be applied, or is it something relative to the subject matter under investigation? Is the logic of discovery, the guiding but nevertheless not absolutely trustworthy logic of the preliminary stages of an investigation, the same for all ranges of subject matter or is it relative to the subject matter of the different sciences? Without attempting to go into a full discussion of the question we may say that the secure logic of demonstration appears to us to be one and the same whatever the field of investigation. The forms of reasoning which in one science compel assent to its conclusions from accepted propositions are the same as those which in any other science have the same compelling power. One form, for instance, mathematical induction, may be rather frequently employed in one science and appear seldom or never in another; but it is valid wherever it applies and has the same compelling power.

But it seems not improbable that a certain heuristic logic in one science may have no conceivable place at all in another.

In some investigations which I have carried out in the past two or three years, I have had occasion to treat a great variety of related transcendental problems by means of methods to which I was led by certain fundamental algebraic guides to transcendental problems. Such guides appear to have been first employed by Sturm (in 1836) and by Cauchy (see Moigno's lectures, 1844). They were

brought into great prominence in more modern times through the initiative of Volterra whose work in this direction first became explicit in his publications in 1896. They appear to be of such a nature as to be useful only in mathematics; and in fact in only a certain well-defined region of mathematics, though their full value here seems not yet to have been realized in accomplished use. It is of interest to note the sort of results to which they give rise. One is led by them to a more definite and precise formulation of a variety of problems originating from certain applications of mathematics to physical phenomena, the formulation being so sharp and clear as to enable the mind to concentrate its thought upon the leading issues and to avoid the waste due to a distraction of attention by irrelevant matters. The central fundamental theorems around which the detailed theory of these problems gravitates are suggested so clearly by the heuristic process to which one is led in an unmistakable manner as to leave no room for a failure to discover these theorems, at least in a wide range of problems. The process does not directly and immediately afford us a proof of the theorems. But it does yield precise suggestions concerning the method of proof by which one may establish them through a rigorous logical procedure. This particular heuristic logic, then, serves the three-fold purpose of making the problems definite, of suggesting the central theorems, and of indicating suitable methods of proof.¹

Perhaps this example represents the extreme of definiteness and serviceability in these heuristic logics. All gradations exist between this and that other in which the inference from the known to the unknown is through well-defined processes which are imbued with full logical rigor

¹ This heuristic logic afforded the principal subject of my retiring address as Chairman of the Chicago Section of the American Mathematical Society in December, 1921; a detailed account of it for mathematicians will be found in that address as published in the *Bulletin* of the Society for April-May, 1922, pp. 179-210.

and by which one may pass directly from the known to the unknown in such a way that in the very passage to the new truth there is inherent the compelling logical demonstration of that truth. Let us for a moment contrast this latter sort of logic of discovery with the former.

We can get it before us best by taking an example where it would naturally be employed. Let us suppose that one has observed that the positive integers may be separated into two classes: in the one class are those positive integers each of which is a product of two smaller positive integers; in the other are all positive integers not in the first class. Let us call the integers of this second class prime numbers. Let us suppose now that one has already found out in some way that every integer of the first class contains as a factor some prime number greater than unity. Suppose then that he raises the question as to the number of integers in each class. In both classes together there is an infinitude of numbers, since these classes together contain all positive integers. That the first class contains an infinite number of integers is obvious, since it contains an infinitude of powers of each integer or since it contains the double of every positive integer. The question which remains and calls for answer is whether the number of primes is infinite. The answer, complete or in part, must evidently be one or the other of the following: the number of primes is finite; the number of primes is infinite. The order of procedure is obviously to assume one or the other of these alternatives and to test it; if we assume the wrong one we can expect to arrive at a contradiction. Let us try out first the simpler assertion that the number of primes is finite. Then let P be the product of all of them, and consider the number $P+1$. It is divisible by no one of the primes except unity, since we have a remainder of one on any such division owing to the fact that P is now supposed to be the product of all prime numbers. Hence we have a

number larger than any prime and without a prime factor, in contradiction with what we already knew. Hence, the number of primes is infinite. Our question is therefore answered with a precision which may be accepted as tentatively satisfying.

Here the process by which we arrive at the answer to our question contains the proof that the answer is correct. Here the logic of discovery is in no wise different from the logic of demonstration.

But there is something peculiar about this case which is not present in all cases. The question whose answer we sought has by its nature one of a finite number of answers which press themselves at once upon the attention as the logical possibilities, and this almost as soon as one has clearly conceived the question. Let us ask, on the other hand, what is the law of force among the atoms or parts of atoms in chemical combination. There is no such finite set of exhaustive and useful logical possibilities to arrest the attention. So far as the logical elements in the situation are involved there is an infinitude of logical possibilities of co-ordinate importance. What we know about the matter is far too little to compel as inference one or the other of any finite set of useful logical possibilities. We cannot proceed to the desired truth by means of logical processes compelling the conclusion and demanding confidence in the results attained. We need some logic of discovery different from that which is suitable in demonstration.

Such a heuristic logic will be necessary partly (and roughly) in proportion to the definiteness and completeness of the underlying truths already in hand and on which we proceed to build the theory. Such necessity will increase with the complexity of the problem to be investigated and the consequent difficulty of an orderly procedure from the known to the unknown. Hence there are two

stages in the development of a science when one will be in an especial need of a heuristic logic. The first of these is that necessary for a science in the nascent stage of its development when its underlying basic principles are being discovered and put in order. This need will subsist in greater or less measure for each experimental science as a whole, and especially in its infancy. The second of these necessities is that which arises in the remote and complex developments of some phase of a science when one wishes to branch off rather widely from the beaten trail and to develop a new chapter or section of the science.

It is not our purpose to consider the logic of discovery outside of the domain of the exact sciences nor indeed to discuss the variety of heuristic logics suited to the various sciences or their several parts except in so far as this may be convenient in analyzing the general character of such logics. From the example which we have exhibited from mathematics it must be clear that a logic of discovery may be special to a particular well-limited class of closely related problems and hardly have a point of contact with any other investigations whatever. Other examples with the same character can be found in mathematics, especially in those fields where the physical intuition can be brought to bear upon the mathematical problem, as in the theory of differential equations (ordinary and partial) with boundary conditions. It seems likely that this relativity of the logic of discovery to the particular subject matter of investigation will be found to be a characteristic of it in all divisions of science.

The tentative nature of the logic of discovery allows room for an error of a dangerous sort. In some cases the measure of sagacity required in the successful use of such a logic is not very great, and the investigator is therefore able to sense accurately a considerable class of results without a need for discarding any. His success for a time is

so uniform that he begins to lose sight of the tentative character of his processes of inference and to think of these as secure in the sense that they guarantee the validity of the conclusions attained. Then he gradually ceases to feel the need of a test of verification and is inclined to be satisfied without it, particularly if it is hard to devise such a test. He begins to have an undue confidence in his heuristic method. He has often found it successful. If it has ever led him astray he has seen clearly where he lacked in sagacity. In the new situation he seems to have avoided all extraneous sources of error. He concludes therefore that the result heuristically attained is valid even though he has not tested it independently.

It appears that an error of this sort is especially likely to arise in those domains of natural science in which one initially makes large abstraction of the actual complexity of the phenomena in order to bring them within the range of successful investigation.

Let us take an example of this, purposely put into extreme form in order to make the point clear.

Let us suppose that one is investigating the processes of thought. He examines all the observable circumstances connected with a process which yields a poem or an hypothesis in natural science or the consequence of some physical law or a theorem in mathematics. Waiving the question, for the moment, as to whether there is something hidden which he cannot see, he proceeds to make a complete catalog of all that he can find as he looks upon the thinker while in the process of thought. He varies the individual under investigation and the circumstances under which he is examined and the subject matter of his meditations. After a time he has a large number of facts of observation. Testing them for common properties, he finds that every one of them is of the nature of a physico-chemical fact. They are the sort of thing which the careless observer

might call the physico-chemical concomitants of the processes of thought. Our investigator is too careful to name them in this way; for that would already be to read into the observed facts a large measure of deep-lying hypothesis, and it is desirable to avoid this lest we read our prejudices into the facts. The subject whose processes of thought are being investigated will not be asked to give an account of his own experience; for, in doing so, his prepossessions will necessarily color his account. The matter must be made more thoroughly objective than would be possible under such a plan of procedure. The observed facts are before the investigator, shorn of everything which might be colored with prepossessions. They are all physico-chemical in their character. In how far may they account for the processes of thought? For each recorded movement of the thought process there is a physico-chemical phenomenon. What is the connection between them? In how far can one describe or explain the processes of thought in terms of these physico-chemical changes? These are natural and legitimate questions.

Let us suppose that a very considerable success has been attained in setting up a definite one-to-one correspondence between isolated items of thought and particular physico-chemical changes, so that one may measure certain movements of the physical frame and tell the subject truly at least a part of what he was thinking at the moment. As the work proceeds the investigator becomes more successful in recording the thoughts by means of the observed reactions of the physical frame. He begins to raise definitely the question as to how far he may go in accounting for thought in terms of physico-chemical changes and (let us say) he begins to incline to the view that a complete success is possible. In his meditations he begins to say to himself that if there is anything in the thought process besides these physico-chemical changes then that part of it

cannot be subjected to scientific investigation. A long experience with the matter inclines him more and more to identify these physico-chemical changes with the thought process which he started out to investigate; and he is led finally to assert the identification.

This case I have purposely made extreme; but I find it difficult to tell how far our psychologists have gone in this direction. Some of them have gone quite far enough to leave me bewildered. They seem to have laid aside some of the fundamental elements in the problem and to have neglected the fact that they have done so. Or have they merely delimited the field of "psychology" and left the study of the mind, in the older sense, to philosophy or to some science not yet created?

If I seem to have departed from my subject of the logic of discovery I wish now to come back to it and to say that it is legitimate to make any tentative abstraction of elements whatever that may seem desirable in a particular investigation, provided that it is always remembered that such abstraction has been made. It is desirable to know in how far the processes of thought can be described and explained in physico-chemical terms. But it is undesirable to allow the success in establishing the correspondences between the two things to obscure the fact that the style of the investigation necessarily leaves out of account entirely a certain type of phenomenon, and that it therefore throws no light on the question of the existence or non-existence of this type of phenomenon.

It has been said that "the greatest discovery ever made in philosophy was that the way to discover whether a thing is present is to look and see." In ancient times the exponent of this doctrine was Aristotle, while Francis Bacon brought it to clear notice in the modern world long after Telesio and Roger Bacon had unsuccessfully insisted upon it. But, as applied to external nature, the doctrine was

not in good repute with Plato who considered it erroneous and upheld as true that which agreed with his sentiments of propriety and beauty. Since the latter were supposed to have been ascertained by looking inward upon the mind this process of introspection finally came into disrepute because it led to contradictions with what was found by looking upon the external world to see. This disrepute of the practice of looking inward has been so great that some psychologists seem to have become afraid to use in their science as a method of discovery the simple one of looking and seeing. Their logic of discovery, so some of them seem to insist, must be one of induction from the observation of physico-chemical phenomena. To an outsider they seem to be in need of finding out again that the way to discover whether a thing is present is to look and see, at least in matters pertaining to the experience of concentrated thought. Their logic of discovery seems to have been made too narrow.

This narrowing of the range of the logic of discovery is not peculiar to any one science. It seems to me to be an ever-present danger in the necessary form of the process of discovery in any natural science. We cannot deal at once with the whole complexity of phenomena. We choose a certain part of them and try to find our explanations in terms of that part. It is always admitted that complete explanations have not been found; but that failure is accounted for by an insidious error common in what I would like to name the proof by ignorance. It is likely to arise when great abstraction has been made and this fact has been ignored. A good example of it in a general situation is afforded in condensed form by the following quotation:

"If, then, it is impossible, through deduction beginning from the transformist hypothesis, to build a theory of morphological evolution verified by experience, this is not be-

cause the hypothesis is false; it is because it is incomplete and corresponds solely to certain factors of evolution. In order to foresee the results it is necessary to know all the factors. The apparent indetermination arises solely from the insufficiency of our knowledge."

"The apparent indetermination arises solely from the insufficiency of our knowledge." Here in a single sentence is the essence of the proof by ignorance which in one form or another is often advanced in scientific discussions and sometimes treated in a dogmatic way widely variant from the spirit of true science. If one is to maintain the method at all he will probably have to do it merely by dogmatic assertion; for there does not appear to be any argument in its favor. It should be apparent to every one, on reflection, that we can never prove any positive proposition, any significant truth, by means of our ignorance of the facts in the case—except the one fact of our ignorance. When it is said in the foregoing statement that "the apparent indetermination arises solely from the insufficiency of our knowledge" the truth of this cannot be known from our knowledge of the facts, for we are confessedly ignorant of them. Three possibilities arise then: either the truth of the statement is not known at all; or it is known by some transcendental insight into external phenomena; or it is known through our ignorance of the facts. There is no room to doubt what the scientific conclusion is in the matter: the statement is not known to be true. It is neither demonstrated logically nor verified experimentally.

Perhaps we should dwell still longer upon the absurdity of this proof by ignorance, for it contains the essence of frequent error which vitiates many conclusions. W. K. Brooks in his *Foundations of Zoology*, truly says: "The hardest of intellectual virtues is philosophic doubt, and the mental vice to which are most prone is our tendency to believe that lack of evidence for an opinion is a reason for

believing something else." In the absence of any evidence that the indetermination is not due to our ignorance we are inclined to conclude that that is the ground of it. The second law of thermodynamics rests on just such insecure foundations. We know no facts to dispute it; we are far from having established it. The present situation warrants our taking it as an hypothesis to see what we can get out of it; but it does not justify any uneasiness of mind as to what it may say about the future history of the universe. The principles of the conservation of energy and of the conservation of mass, or their modern combination into a single principle, may well serve as a working hypothesis. But we must remember that no conclusion based on an hypothesis of such a character carries with it its own validity. Such a logic of discovery requires to be supplemented by an independent test of the result; and the latter may be accepted only provisionally in the absence of such a test.

There is something finer in a possible logic of discovery than anything which we have so far made explicit. One may arrive at truth not only by induction and deduction but also under the impulse to realize directly an ideal as to the form of the truth to be attained. The most striking recent instance of this is found in the general theory of relativity as developed by A. Einstein. In order to bring out clearly its character in this case we shall have to present certain elementary considerations associated with one aspect of the theory of relativity.²

Let us approach the matter by thinking of a geometrical curve fixed in the space interior to a given room of four walls meeting at right angles. If we take the floor and two adjacent walls to be a system of reference by means of which to locate the positions of points in the room,

² By taking the technical mathematical terms in their usual non-technical sense the non-mathematical reader will have a sufficiently clear idea to make the argument intelligible.

then we can uniquely define the position of a point on our curve by giving its distance from the floor and from each of the two walls selected. If the point moves along the given curve then the numbers expressing these distances will vary and will be related according to a law determined by the shape and position of the curve; these three variable numbers will satisfy certain equations of condition. Now suppose that we modify our procedure by using the ceiling and the other two walls as a system of reference. Since the relation of the curve to this system of reference is in general different from that to the former we obtain in general different equations of condition for the same curve. These conditions for the same curve would be modified still further if we should choose some other set of three mutually perpendicular planes for the system of reference, and especially so if these planes should be oriented in some new directions.

It is clear that the properties of the curve itself have in no wise been affected by these changes in the system of reference, even though we have several times modified the mathematical expressions by means of which these properties may be most compactly and most completely described. Let us for a moment forget these systems of reference and consider the curve itself by passing along it from point to point. Two characteristics will force themselves upon our attention: The amount of bending of the curve as we pass along it, its curvature; the amount of twisting of the curve, its torsion. These are intrinsic properties of the curve itself, capable of representation at each point on it by definite numerical values. These numerical values can be expressed in terms of the three distances pertaining to any given one of the systems of reference mentioned above; it turns out that definite rather simple formulae exist for expressing the curvature and torsion in terms of the named measurements. Since these describe

intrinsic properties of the curve their values must be unaltered by the transformations of variables due to the changes in the system of reference; that is, they must be invariants of the transformation.

Thus it is seen that the analytic expressions for the curvature and torsion are unchanged in form and in value as we pass from one of our systems of reference to another. It can be shown that they completely determine the intrinsic properties of the curve. Then we have in them a complete mathematical description of the intrinsic properties of the curve in a form from which we have abstracted those peculiarities which belong to the special system of reference by means of which we described the curve and its position in the first place. This sort of abstraction is of frequent and important use in mathematical investigations. It affords one of our methods of excluding from consideration those things which are irrelevant to the central purpose of the investigation and of fixing attention upon those things alone which are unaltered by, or are invariant under, the transformations permissible among the elements in consideration. A similar but extended use of invariants gives substance to the ideal which guided the development of the general theory of relativity.

Two considerable extensions are necessary before we can realize precisely the situation in the development of the Einstein theory. The first has to do with a generalization of the system of reference. We must replace the three mutually perpendicular planes of our system of reference by three warped surfaces, perhaps twisted and corrugated and irregular in shape and restricted only enough to allow us to utilize them successfully for the unique location of points in space. By means of these we are to describe the space configurations with which we have to deal. The other extension consists of the introduction of time into our system. We cannot well develop the mechanics of

three dimensions by means of what is merely geometric in three dimensions; but if we introduce time and think of our space-time continuum as affording a four-dimensional world, then our mechanics in three dimensions is replaced by a geometry in four dimensions. In the Einstein theory one no longer tries to maintain the separation of measured space and time; they are not independent; they are indissolubly united into a four-fold space-time extension. In this space-time of four dimensions we are to choose as a system of reference four warped three-dimensional spaces by means of which the location of points in this four-dimensional space-time shall be defined. We can go from one such system of reference to another by a change of fundamental variables. The totality of these changes is said to form a group, and certain subsets of them are called subgroups.

With these conceptions in mind, it is easy to make clear the nature of the ideal upon which Einstein insists as to the character of the laws of nature. He wishes to have them expressed in such form with respect to this four-dimensional continuum that there shall be no change in the form of these laws when we pass from one of these systems of reference to another, the mathematical relations expressing the laws are to be invariant when all quantities involved are changed in accordance with a transformation from one system of reference to another; let us say for convenience that the laws are to be stated in covariant form. When we have put them into such form we have abstracted from the statement of them everything which pertains to the particular system of reference employed.

It is a grave question whether the laws of nature are capable of formulation in accordance with the requirements of such an ideal; and an affirmative answer can be maintained only after a searching examination. All precise

evidence which exists up to the present time is in favor of the conclusion that such an ideal may be realized.

It is not necessary to our present purpose to go into a further analysis of the question as to whether this ideal may be realized in practice. We wish to look upon it as affording an example of a logic of discovery. One here sets up a certain ideal as to the form of the laws of nature. He then takes those known laws which agree closely with experimental facts and enquires whether their statement meets this ideal. This affords the best way to make trial of the validity of the character of the law which his ideal would impose upon nature. If the law as previously conceived meets this ideal there is a certain satisfaction, but there is nothing further to be done with this law. The investigator passes to another in order to discover, if possible, one which is not yet subject to this ideal. Suppose that he finds one, as Einstein did in the case of the Newtonian law of gravitation. It turned out that this law does not accord with the ideal of covariance of the laws of nature. Shall one then give up the ideal on the ground that the Newtonian law is so well established that any deviation from it required by a new ideal shows that this ideal is not realizable? No, not on this evidence alone; it may be, after all, that the law of Newton is not exact and that some modification of it will bring it into covariant form without disturbing its agreement with observed facts. If so, there is likely to be some range of facts, perhaps not previously observed, in which the two laws will give measurably different results; and one will then have a crucial experiment by means of which to discard one in favor of the other.

As a matter of fact, the Einstein theory appears to have triumphed over the Newtonian theory in precisely this way. Whether it has or not is not essential to our present purpose. We are concerned with the heuristic logic in-

volved in the process. In accordance with the Einstein demand it is desirable to enforce upon nature, if possible, a certain ideal as to the mathematical form of the statement of the laws of nature. This ideal guides one's investigations and leads to conclusions as to laws of experimental phenomena. It does not prove these laws; for we have not yet any means of knowing that the laws of nature are capable of expression in covariant form. But it does give us certain new laws, or modified forms of old laws, which we would probably not have reached except under the guidance of such an ideal. The ideal thus gives rise to a logic of discovery. The supposed law once attained is subjected to a searching test. If it survives under this test, then we have a veritable advance brought about by the guidance of an ideal which affords a heuristic means of inferring the unknown from the previously known.

According to this aesthetically satisfying ideal of Einstein, then, we are to have in the mathematical form of the laws of nature a complete covariance under the general group of transformations of coordinates in the four-dimensional space-time continuum. But in the complete realization of this ideal there are certain difficulties of the nature of mathematical complication the avoidance of which would be welcome. The question, then, arises naturally as to whether we might not take certain subgroups of the general group of transformations and apply these to the separate fields for the purpose of obtaining approximate laws—laws which are covariant under the subgroup applicable to a given field or under several subgroups of a given type. Perhaps one may not have as strong expectancy of the validity of such a law as of one which is wholly

covariant; nevertheless one may naturally expect in this way to make closer approximations than he would make without this aid.

This is a new sort of approximation in theoretical physics of a much more profound character than numerical approximation. It is an approximation in the sense demanded by covariance under a subgroup instead of under the whole group. As the subgroup is enlarged, and the statement of the law undergoes consequent modification, the approximation will presumably become closer and may even become as close as is needed for agreement with experiment even though the entire group of transformations is not employed.

The group of the Lorentz-Einstein transformations of the special theory of relativity is precisely such a subgroup as we have just described; and it is well known how it has led to more satisfactory theories of certain phenomena than those which had preceded them. Under this group the Maxwell-Hertz electrodynamic equations and the wave equation deduced from them are invariant. Now, if all the phenomena in a certain field are invariant under this transformation group then every mathematical formulation of law in this field should be in the form of an equation which is covariant under this group. This would afford a precious guide as to the necessary form of such an equation. If an equation is obtained in an empirical way, then one considers it only as a rough approximation to the true equation; and one seeks a better form of it which shall have the two properties of being invariant under the named group and of being represented to a suitable approximation by the empirical equation first obtained.

It seems not unlikely that each large and well-defined class of phenomena may have associated with it a certain group of transformations—a subgroup of the general group of transformations of axes in the four-dimensional space-time continuum—of such sort that the phenomena in question (or at least the most of them) may be represented to an approximation quite within the range of experimental error by requiring merely that modification of approximate empirical laws which is required to bring them into a form that shall be covariant under the given subgroup of the total Einstein group.

This procedure might well have the advantage that it may be carried out much more readily than the corresponding one based on the original group, at least if the subgroup is of simple character, say analogous to the group of all multiplicative linear homogeneous transformations.

From certain mathematical considerations it seems probable that in many important cases one would actually obtain in this way precise laws the covariance of whose statement under the general Einstein group could be readily established. The number of invariants relative to a given subgroup and agreeing with a given rough empirical law to a suitable approximation may often be small or be even unity. In the latter case it must be the law which is covariant under the general Einstein group, if there is such a law. In the former case, one would probably have little difficulty in choosing the appropriate one for general covariance (if such a one exists).

Thus it seems likely that a systematic study of the covariance of laws under subgroups of the general Einstein group will lead to useful means of discovering the

laws of phenomena. Thus for each of these subgroups we appear to have the possibility of a logic of discovery of the same general sort as that employed by Einstein in his general theory and differing from the latter only by virtue of its being relative to a subgroup of the Einstein group instead of to the whole group. Thus it seems probable that we may have in physical science a considerable variety of logics of discovery based on transformation groups as the ultimate ground under the ideal as to the form of statement of the laws. Of course, every law obtained in this way (as well as in any other) must be subjected to experimental test before it can be accepted. The method would profess only to help in discovery. Some of the results to which it would lead would turn out to be valid (if the general theory of relativity is valid) and others would need further modification under the guidance of a more comprehensive group.

Let us give a brief summary of the foregoing discussion:

It is conceivable that in the nature of things the mind is unable to comprehend its highest movements with clearness and also that certain of its acts of discovery are essentially creative in character; but neither of these possibilities need affect the development of a logic of discovery in the sense of a logic by which one infers from the known to that unknown which hitherto has not been apprehended or suspected. Such a logic may lead forward by well-defined steps to clearly ascertainable propositions without carrying with it a demonstration of those propositions. The already developed systematic logic is a logic of demonstration. The logic of discovery, in the higher sense of

the term, has no existence as an actually developed science. This was insisted upon in effect by Descartes and Francis Bacon and remains as true in our day as in theirs. Bacon's supreme effort to found a logic of discovery ended in failure. No single discovery can be pointed to which can be definitely ascribed to the use of his rules.

There are two causes which may have contributed to this failure. It is conceivable that the primary acts of discovery are so essentially creative in their character that no science of inference from the known to the unknown can be developed; or, if it can be developed in part, that it cannot be adequate except in a very restricted range. This does not seem to have been the main difficulty. The conception of the logic of discovery as a unit, comparable in unitary character to the logic of demonstration and scarcely separable into parts, has, I believe, been a chief hindrance to its development. We can have not so much a logic of discovery as logics of discovery each relative to some field or subject matter of investigation. Discovery is relative to the point of view. We can no longer proceed as Descartes did when he "thought out what the constitution of the world and man must be if they were to be clearly understood."

The essential step of discovery is in the formation of a conjectured proposition to be tested or of an hypothesis out of which the proposition comes or of an ideal which guides in the formation of both hypothesis and proposition. It is possible that truth may be discovered by means of a logic which carries with it the demonstration of the truth; but the more characteristic logics of discovery are merely suggestive in character. The former is often illus-

strated in mathematics, particularly in the theory of numbers and the theory of finite groups. In physical science it is the latter which is generally in evidence. This latter also has a wide usefulness in mathematics. Every result of a heuristic logic must be subjected to some suitable adequate test after it is obtained. In mathematics it is often demonstrated by a *de novo* argument. Such a heuristic logic serves the three-fold purpose of making the problems definite, of suggesting the central theorems of an investigation, and of indicating suitable methods of proof.

The tentative nature of logics of discovery allows room for an error of a dangerous sort. When the measure of sagacity required for the use of such a logic is not great one may so uniformly succeed with it for a time as to lose his sense of the need of an independent test of the result. By hidden gradations of error one may then pass step by step to the condition of being satisfied with the unsound "proof by ignorance" so that he is able to conclude an argument with the absurd climax: "The apparent indetermination arises solely from the insufficiency of our knowledge."

The finest thing associated with a logic of discovery is that one may arrive at truth under the impulse to realize directly an ideal of far-reaching importance as to the form of the truth to be attained. The most recent striking instance of this is to be found in the general theory of relativity. In accordance with Einstein's demand it is desirable to enforce upon nature, if possible, a certain ideal as to the mathematical form of the statement of the laws of nature. This ideal guides investigation and leads to new laws. It affords a heuristic logic associated with a general

group of transformations in space of four dimensions. It seems probable that certain related but more special logics of discovery of distinct usefulness may be associated similarly with certain subgroups of this general group and that we may thus have in physical science a considerable variety of logics of discovery each based on a transformation group as the ultimate substance of a related ideal as to the form of statement of the laws.

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